perience and fund of illustration possessed by Mr. Darwin, and I have to say that I am unconvinced by the arguments he has adduced. With the transparent frankness of all his writings, Mr. Darwin, in one of the references to which he has commended me, has collected a very large number of examples that tell very strongly against him, and which I again commend to Mr. Wallace. I refer to the 18th chapter of Mr. Darwin's book on the "Variation of Plants and Animals under Domestication," and especially to that portion beginning on page 149. In speaking of animals, he says:—"The most remarkable cases, however, are afforded by animals kept in their native country, which, though perfectly tamed, quite healthy, and allowed some freedom, are absolutely incapable of breeding. Rengger, who in Paraguay particularly attended to this subject, specifies six quadrupeds in this condition, and he mentions two or three others which most rarely breed. Mr. Bates, in his admirable work on the Amazons, strongly insists on similar cases, and he remarks that the fact of thoroughly tamed wild animals and birds not breeding when kept by the Indians, cannot be wholly accounted for by their negligence or indifference, for the turkey is valued by them, and the fowl has been adopted by the remotest tribes. In almost every part of the world, for instance, in the interior of Africa, and in several of the Polynesian islands, the natives are extremely fond of taming the indigenous quadrupeds and birds, but they rarely or never succeed in getting them to breed," and so on, through sixty pages of closely-packed examples. And what is Mr. Darwin's commentary on these facts? I again quote page 158:—"We feel at first naturally inclined to attribute the result to loss of health, or at least to loss of vigour, but this view can hardly be admitted when we reflect how healthy, long-lived, and vigorous many animals are under captivity, such as parrots and hawks when used for hawking, chetahs when used for hunting, and elephants. The reproductive organs themselves are not diseased, and the diseases from which animals in menageries usually perish are not those which in any way affect their fertility. No domestic animal is more subject to disease than the sheep, yet it is remarkably fertile." Mr. Darwin, with equal clearness and conclusiveness, decides that this sterility cannot be due to a failure of sexual instincts, change of climate or of food, or want of food or exercise; and he concludes that certain changes of habits and of life affect in an *inexplicable manner* the powers of reproduction. But what is true of man it is reasonable to suppose is true of all these instances—namely, that it is a more luxurious habit, a more vigorous health, a less precarious existence, induced by the care and attention of domesticators, that have caused the sterility; that these animals are too well off, and not that they are ill off in any way; and this theory explains the whole most conclusively. On the other hand, and in opposition to this vast and uniform collection of examples, Mr. Darwin adduces a few instances which tell the other way, but they are very few in number, and seem to me explicable on other grounds. Ferrets, it is notorious, are always kept in a state of extreme depletion and as thin as possible. Domestic poultry are fed almost entirely on poor vegetable food, while their wild and semi-wild relatives feed much more on worms, insects, and on animal diet generally. In regard to sheep, it is notorious that very weak ewes generally bear twins, that Somersets and Dorsets are more fertile than Southdowns and Leicesters. We have, I may add, no facts to guide us in regard to wild dogs, and few in regard to wild cats; but we do know that in tame ones the half-fed lantern-ribbed curs are more prolific than their sleek relations. In regard to domestic fowls, and especially pigeons, we must remember that their condition is materially altered by the disuse or only very partial and irregular use of their powers of flight, this must reduce their circulation and vigour very considerably, and make them *pro tanto* so much weaker. But these instances, upon which Mr. Darwin relies to answer Doubleday and others, are very partial indeed. In his own pages, as I have already said, they form a very small element compared with the overwhelming cases he quotes on the other side. So much so, indeed, that these cases may be taken as exceptions which prove the rule that domestication and improved conditions of life induce sterility in animals.

It savours of scholastic philosophy to speak of Nature as exercising any influence on the regeneration of races, and yet there may be sound philosophy in the old notion that when an individual or a class is in danger of being extinguished from want, Nature puts forward a special effort to preserve it. The sickly mother, the half-starved plant, is more likely to breed than the healthy and the vigorous. If we remove the peasant's family to the drawing room, it will cease to be composed of ten and twelve children. If we remove our daisies and cowslips to the greenhouse, their flowers grow double, and they ripen no seeds. The vine that has felt the frost is the one to pay the rent. Wherever we turn, in fact, we meet with examples of the universal law; and this law seems to be at issue with an important portion of Mr. Darwin's theory, namely, that in the struggle for existence, the vigorous, the hearty, and the well-to-do, elbow the weak and decrepid until they elbow them out of existence, and supplant them. If I have said anything above which can be construed into an impertinence, I unconditionally withdraw it. The only excuse for soreness, is an impatience at what seems to the writer to be indefensible dogmatism. The days will not be ripe for scientific dogmatism until the Infallibility of Positive Philosophers has been generally accepted, and it does not do to forestal that millennium.

H. HOWORTH

MR. WALLACE has effectually set aside Mr. Howorth's new views on Darwinism, and it now only remains to point out that the latter gentleman, in his instances, puts the cart before the horse. Hens that are fat and don't lay are fat because they don't lay. When the sexual powers, either in plants or animals, are defective from accident or design, the overgrowth always takes place, and this among animals is chiefly by the increase of adipose tissue.

Birmingham LAWSON TAIT

Recent Neologisms

I HAVE been long accustomed to register the first appearance of new words and phrases. Of course the vast majority of these take no root, perishing where they fall. Here is a sample of the latest issue: *Survival*, introduced, I think, by Darwin; *indiscipline* and *impolicy*, which were brought in by the Franco-Prussian War, and also the vulgarism *to telegram*. The greatest atrocities in this line are committed by "physicists," if the shade of Faraday will pardon me the use of that word; and far away the worst coinage I ever encountered is due to Mr. Alfred R. Wallace. As it is "meet and right and our bounden duty" to stigmatise such intruders, and if possible prevent their adoption, I take the liberty of making my feeble protest against Mr. Wallace's "prolificness," which he introduces to our notice in his letter on Mr. Howorth (NATURE, July 6, 1871, p. 181). In this case the hideousness of the coinage is some guarantee against its reception.

Malvern Wells, July 8 C. M. INGLEBY

Affinities of the Sponges

I HAVE just read with much interest the paper in NATURE by Mr. W. Saville Kent, criticising my friend Carter's article in the "Annals of Natural History" for this month, in which I fully concur. How Mr. Carter can have fallen into such an error, for such I must call it, I cannot imagine, as comparing a group of animals in Botryllus to those sponge cells, even in so highly a developed form as Grantia. For, taking this as the highest known form of sponge animal, it is at most only a monociliated sac, as shown both by Prof. Clark and by Mr. Carter. Now, it is well known to all investigators, and Mr. Carter has shown it himself, that the animals of Botryllus have distinct oral and fæcal apertures, whereas the sponge cell, so far as has yet been seen, has only an oral aperture. Again, the Ascidian Botryllus is shown to be far higher in the scale when we come to compare its internal organisation, and not merely to confine ourselves to the sac-like tunic. The discharge of the fæcal matter into a common cloacal canal is to me not a sufficient reason for comparing these groups of animals to the sponge animals in *Grantia*.

But what I wish to draw attention to more particularly is this, that in the hurry and bustle of our investigators of the present day, all old associations are mostly, if not entirely, forgotten. I can scarcely think that they are ignored, but are forgotten. Thus, Prof. Grant was, I believe, the first to determine the character and the full importance of the seed-like body in *Halichondria* by placing watch-glasses in the vessel in which living specimens of the above sponge was placed; the bodies were thus discharged from the fæcal canal of the parent sponge, and attached themselves to the watch-glasses, and he then carefully watched their development. Mr. Carter, being a pupil of Dr. Grant, no doubt followed his teacher's plan of investigation, which has led to the brilliant results of this gentleman's in-

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vestigations of the fresh-water species in the tanks at Bombay. The clear and lucid manner of investigation detailed by Prof. Grant in the *Edinburgh New Philosophical Journal* (1826-27) might be held as a pattern for investigators, but he appears to be almost entirely lost sight of.

Again, as regards the animals of *Grantia compressa*, Prof. Reay Greene certainly preceded both Prof. Clark and Mr. Carter in his investigations, and has figured these monociliated animals in his handbook, published in 1859, p. 31, fig. 6. The figures are on the same scale as those given by Mr. Carter, and indeed some of the groups figured are so much like those given by Mr. Carter in the "Annals," pl. 1, sqr. 13, *a*, *g*, *h*, that it would be difficult to separate them, and the same may be said of Fig. 41 of Prof. Clark's in "Ann. Nat. Hist" pl. 6, 1868. The only difference being the want of the funnel-shaped mouth, which seems to have escaped the observation of Prof. Greene, probably owing to want of definition in the instrument used in the investigation. Now there is an amount of credit due to the first demonstrator of these animals, which, so far as I have seen, does not appear to have been accorded to him; and I therefore take the liberty of directing attention to this fact. I do not know Prof. Greene, and therefore do not take up this matter on personal grounds, but only in fairness due from one scientific man to another, and I hope my friend Carter will take this in the spirit it is intended. EDWARD PARFITT

Exeter, July 8

Cramming for Examinations

I ENCLOSE one or two *bonâ fide* extracts from "Middle Class" examination papers which have during the past few weeks come under my notice officially.

I do not wish thereby to reflect so much on the candidates as upon the mode of teaching in Middle Class schools, which produces such results.

As might be expected, where evidence of "cramming" from a text-book and want of practical knowledge are equally manifest, some of the answers in the papers from which these are selected are pretty good—but what can be the real value of knowledge of this sort?

The questions are sufficiently indicated by the answers.

CANDIDATE A.

Chlorine may be taken from decayed vegetable matter and animal matter, also manure. . . . It is used for killing insects, it is compounded with lime, and is very good when compounded with lime for the manuring of fields. Lime is chiefly formed from Chlorine.

CANDIDATE B.

Chlorine is prepared by mixing $Ca\,Cl_2$ with H_2O

$$Ca\,Cl_2 + H_2O = CaO + H_2 + Cl_2$$

Chlorine is a colourless invisible gas. Has no odour nor taste. . . . Hydrochloric acid is prepared as follows—

$$Ca\,Cl_2 + H_2O = CaO + 2\,H\,Cl$$

CANDIDATE C.

Carbon is an elementary substance, it is one of the constituents of the atmosphere, it is found in lime and pits among the coal. When the lime is soaked with water the carbon escapes out and the lime moulds away.

AN EXAMINER

Great Heat in Iceland during the present Summer

MR. R. M. SMITH has received a note from Dr. Hjaltelin, Corresponding Member of the Scottish Meteorological Society at Reykjavik, dated June 30, of which the following is an extract:—

"We have now the most excellent season you can imagine in these latitudes, the average temperature for this month (June) being as high as 59°, which is 12° higher than the mean temperature of the past four Junes. I was yesterday near the Hengil Mountain, just at that place where we pitched our tent last time you were here, and the heat was quite unsupportable in the valleys. The wind has been continually blowing from the south-west. Some Englishmen setting out for the Geyser will have something to tell of the extraordinary heat we have at present."

ALEXANDER BUCHAN

The Late Thunderstorm

AN ash tree in the garden attached to the farmhouse of Wester Cringate, near Fintry, struck by lightning on the 20th of June, presents a singular appearance.

About 20ft. from the ground a large branch has been torn from the trunk. The bark has been neatly peeled off for a few feet above and below the place from which the branch shot out. The wood has been first struck a little above the branch, and shows a clean cut, such as might have been made by a sharp-edged tool, as if a chisel three inches broad had been driven into the wood for about four inches. The branch itself has been torn, not cut, and a stripe of the trunk about two feet long below the branch has also been torn out.

For the next four or five feet the tree has suffered no damage of any kind, but after that space the trunk bears six parallel downward scars, varying in length from two to five feet. The scars do not all begin or end at the same height, although each might be cut in some point by a horizontal plane passing through the tree. They spread over about half the circumference of the trunk, and can all, or nearly all, be seen from one standpoint. The most striking circumstance, however, is the almost perfect parallelism of the scars, which are not vertical, but a little twisted round the trunk like the rifling of an Armstrong gun, the rifling in this case being on the outside of the barrel. Six chisels of about half an inch in breadth seem to have ploughed into the wood, tearing off at the same time rather broader stripes of the bark. Towards their lower ends the three right-hand scars cease to be quite parallel, and tend to converge; but all three die out before the convergence takes place, and the tree for the next two feet or so is unscathed. Five feet from the ground (at about the point at which the three scars would converge, if produced) a single rut cutting deeply into the wood commences, which continues down to the soil.

The garden wall (which is a "dry-stone dyke," *i.e.* of loose uncemented stones) passes some three feet behind the tree, on the side directly opposite to that on which the markings above described occur. Outside of this garden the lightning has ploughed two pretty deep parallel ruts through the grassy soil some four feet apart, and stretching from the foot of the wall to the edge of a ditch, a distance of three feet. These ruts are the last observable traces of the passage of the lightning, and were probably made by the currents which engraved the three left-hand scars on the tree. Of course it is impossible to decide whether the currents passed through the open wall, or down the outside of it.

Three sheep on the neighbouring farm of Spittalhill were killed in the same thunderstorm. Their carcases were found lying in a line and were very much swollen, but bore no external marks of injury. A small patch of wool had been stripped from the flank of one of them, but probably this had no connection with the cause of death. R. L. JACK

Geological Survey, Fintry by Glasgow, July 5

Saturn's Rings

As you have favoured my work on "Saturn's Rings and the Sun" with criticism, I feel sure that as that criticism is adverse to my views, you will in fairness allow me to reply to it.

I will do so in detail. Your reviewer commences very much under the impression that Prof. Clerk Maxwell having investigated the "Stability of Saturn's Rings," no one else is to venture into any discussion touching on their nature or origin. In fact he issues a *caveat*—Prof. Clerk Maxwell has concluded the subject! Next he asserts that I have not seen the Professor's work, because I ascribe to the perusal of Mr. Proctor's "Saturn and its System," the enlistment of my "interest in favour of the Satellite Theory." This is surely beyond his province, as I am free to choose my own point of starting. Mr. Proctor's work interested me, and so did Mr. Clerk Maxwell's, but the former elicited my work, the latter did not.

He next accuses me of placing too great faith in figures, and shows surprise at my giving the hourly rate of the solar motion to a mile, and the solar parallax to four places of decimals. The solar motion is that given by the Herschels, and the solar parallax by several observers. He is hard to please. But my reviewer has unfortunately missed the point of my arguments. The actual velocity of this solar motion is perfectly immaterial; indeed, had he followed the reasoning, he would have seen how pointless are his objections.

As regards my arguments in favour of the meteoric theory of the sun, the reviewer is equally inaccurate. As to my being

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